AOARD REPORT

'93 Workshop on End Stage Transition, Syracuse, New York

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Syracuse University/Minnowbrook

This report summarizes highlights of a recent trip by Dr. Roddam Narashima of the Indian Institute of Science in Banglore, India to participate in the '93 Workshop on End Stage Transition, in Syracuse, New York . The workshop was unique in that it brought together, for the first time, those working on the theoretical aspects of transition phenomenon with those concerned with modeling transitional boundary layers in applications, especially in turbomachinery. Dr. Narashima was supported, in part, by the AOARD Window on Science Program.

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WORKSHOP ON END-STAGE TRANSITION

SYRACUSE UNIVERSITY/MINNOWBROOK, 15-18 AUGUST 1993

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Nearly 40 invited participants gathered on the evening of 15 August in the Minnowbrook Conference Centre on the shores of Blue Mountain Lake in the Adirondocks to discuss for the next two and a half days the late stages of transition in boundary layers. The Conference Centre, run by Syracuse University, provided the ideal ambience for an informal and stimulating workshop on a subject that is at once scientifically challenging and technologically important. The Workshop was unique in that it brought together, for the first time, those working on improving our understanding of transition phenomena with others concerned with handling and modelling transitional boundary layers in applications, especially in turbomachinery. Or, as Paul Gostelow (Sydney, who organized the meeting so effectively with John Lagraffe of Syracuse and Terry Jones of Oxford) put it, the intention was to bring the JFM and ASME types together (he mentioned high and low church as well!).

The point was pursued by Roddam Narasimha (Bangalore) in his opening talk (his theme was The Many Worlds of Transition Research). Transition is a complex subject, and its investigation is being pursued in many sub-communities, respectively concerned with stability, receptivity, breakdown, turbulent spots, modelling, direct numerical simulation (DNS) etc. It is remarkable that there is not a single experimental investigation

of transition from laminar to turbulent flow in a boundary layer that goes the whole way - on any one of the many routes that everybody admits are possible.

The Workshop was dominated by discussions on turbulent spots - their genesis, properties and consequences - although Tom Corke (Illinois) described how transition could occur <u>without</u> spots. This spot-less or "scenic" route (as Narasimha called it - the road here is slow and long), discovered by the Novosibirsk group, was followed when a resonant interaction between a TS wave and a pair of oblique waves is arranged (- this is done, in Corke's experiment, by suitably programmed forcing through surface films excited thermally). There is in this case no evidence of rapid collapse into turbulent bursts, but only a gradual filling up of the spectrum. Although Corke finds that the route is followed even when there is some detuning, Narasimha felt that the spot-less route seemed contrived, and Mark Morkovin (Illinois Institute of Technology) questioned whether the disturbance field necessary was "environmentally realisable". Indeed, Morkovin emphasized that while the roads to turbulence are highly non-unique, the modes in which the final onset of bursting occurs are much less so.

Some very interesting discussion took place on the events leading to the birth of a spot. Jim Kendall (JPL) reported that the response of the boundary layer to weak free-stream turbulence (FST henceforth) - created in his tunnel by an array of 168 small jets directed upwind in the settling chamber - took three distinct forms. There is first of all a narrow Klebanoff mode peaking half

way across the boundary layer (- not to be confused with the peak-valley splitting also associated with his name); then there are wave packets, arising sporadically, narrower than Gaster's wave packet but with higher, randomly varying amplitude. Finally there are the TS waves. The dynamics of Kendall's wave packet is not clear. Mike Gaster (Cambridge), on the other hand, could track the birth of what appeared to be an Emmons spot within <u>his</u> type of wave packet, triggered by a surface-mounted microphone. Even when the input to the microphone is (deterministic) white noise, a modulated TS wave train appears, with a subsequent breakdown into a spot. Both experiments demonstrate incidentally how choosy the boundary layer is in picking out waves from whatever hash may be heaped on it.

While Gaster uses singular value decomposition and wavelet transforms to understand the time-frequency structure of the process of spot birth, a 3D view was presented by the flow visualization studies of Chuck Smith (Lehigh), who starts with a single hair-pin vortex generated by fluid injection at the surface. The key to the growth process, in his view, is strong vortex-surface interaction, which leads to more hair-pin vortices that amalgamate into a spot. Bart Singer (NASA Langley) could simulate similar primary and secondary vortices on the computer, but there is evidence of a counter-rotating wall vortex as well in his DNS results: the spot appears only after a rather long gestation period. Seifert (Tel Aviv) reported that two point disturbances separated spanwise, generated using Gaster's technique, interacted to <u>promote</u> transition.

Herbert's (Ohio State) parabolised stability equations (PSE) now take only 30 min. on a work-station and so have potential for engineering calculations. As they are in excellent agreement with both experiment and Navier-Stokes solutions, they can provide flow to the late stages of transition before onset at modest cost; DNS can take off from there on. Encouraged by this experience PSE methodology has now been extended to 3D boundary layers in curvilinear coordinates from low to hypersonic speeds, in both linear and nonlinear regimes. The message was that if the disturbance can be specified properly, and the mean flow is known sufficiently accurately, PSE can take one quite far.

There was much discussion, incidentally, of the possibility of "instability without eigenvalues", the title of a recent paper by Trefethen and coworkers (Science, 3 July 1993). Morkovin traced the history of related ideas; his assessment was that they were not particularly relevant to boundary layer transition, as the implied disturbances were again not "environmentally realizable".

How close to breakdown has basic theory been able to take us? Narasimha, in his opening remarks, was the only one to raise the question of the possible relevance of nonlinear dynamical-system theory to transition. He noted some broad similarities between a set of model equations he had studied with Bhat and Wiggins, and a set proposed by Herbert. These models do not of course reflect the full complexity of the 4-dimensional (3 space + time) forced nonlinear oscillator that the boundary layer is, so we cannot expect to encounter, in real-life transition, the relatively

simple routes to chaos so familiar in dynamical system theory. Although no insight of predictive value could be said to have come from these developments yet, he thought it would be surprising if dynamical chaos had nothing to do with transition. The data analysis methods being developed by Gaster's group could throw light on the problem in coming years.

In any case, according to Narasimha, all the work being done on the different routes to turbulence would eventually have to be codified into appropriate basins of attraction, especially in disturbance space. In general the number of dimensions involved would be too high, but he made proposals for simpler sets of experiments that could help define the concept. Morkovin felt that the number of dimensions would be 10 to 20, and doubted whether the resources - funds, man-power - would be available for undertaking such a task. He was generally pessimistic about the usefulness of the dynamical system approach. Frank Smith (London) described the nonlinear theories currently being pursued, falling into three classes: vortex-wave interactions (important at low input amplitudes), pressure-displacement interactions (medium input) and Euler-scale motions (high input). These theories are very suggestive, and direct comparisons with experimental data in channel flow is encouraging; nevertheless, predictions for flows of the type presented at the Workshop still do not appear to be an immediate possibility.

Wygnanski reviewed what is known about the structure and propagation of fully developed spots. Beyond a critical Reynolds number the TS waves that trail the wing tips of a spot can break

down, creating either apparently autonomous spots or "spotlets" that eventually merge with the parent and so make it grow: he found no support for the alternative view of spot growth, namely that it consists of lambda vortices whose numbers grow in proportion to the size of the spot. It is now well-known that favourable pressure gradients inhibit spot growth - possibly because the wing-tip TS waves are suppressed. Further evidence of this inhibition was presented by Terry Jones (Oxford). Gostelow finds that in adverse pressure gradients, on the other hand, the turbulent region of a spot can spread at a half-angle of 20 degrees, and the attendant wave packet at 29 degrees! Narasimha showed that flow distortion - i.e. a situation in which the streamlines are not parallel but there is no pressure gradient - results in a curved spot trajectory, with spread rates not significantly altered; however the spot is asymmetrical, being thicker on the outside of the bend and not spreading across streamlines necessarily. It is gratifying that spots are now beginning to be studied in more realistic environments, but clearly there may be many surprises in store as other situations are investigated. It is however important to remind ourselves that anomalous propagation is not read into a situation where the spot is not yet mature: the long gestation periods at low Reynolds numbers, known from the early Schubauer-Klebanoff work, can cause confusion in the interpretation of data if one is not careful.

In the real environment of applications, where are spots actually born? According to Morkovin locations are determined by sporadic extrema of the disturbances, but waves and wave packets act as mediators, at least under not highly disturbed conditions.

Narasimha hypothesises concentrated breakdown, i.e. most spots are born in a relatively narrow band around transition onset. The resulting universal intermittency distribution finds support from many measurements, including those presented at the Workshop by Fraser, Gostelow (in pressure gradients as well), Jones, Malkiel & Mayle (in a separation bubble, for the streamwise variation of the maximum value of the intermittency cross-stream, which occurs at the maximum vorticity point), among others. Ian Poll (Manchester) appeared to think that these distributions were being subjected to a rather heavy normalization process, but he also finds use for the same distributions in flow past swept wings, although his "intermittency" is not necessarily the fraction of time that flow is turbulent, as it is for all others, but what one may call a transition progression index, which may be different for different parameters such as skin friction, heat transfer, boundary layer thickness etc.

There is however no direct evidence for the hypothesis of concentrated breakdown, and Hodson (Cambridge) and Jones announced an Oxbridge "spot-hunting" project, which now appears feasible with the development of liquid crystal and multi-gauge surface heat film probes that Jones described at the Workshop. Such a project should in principle be able to track down each spot as it is born and as it propagates downstream. Spot-hunting should be a particularly enjoyable and rewarding exercise in pressure gradients so strong that intermittency distributions are not universal. The reason could be (as Narasimha has suggested) that growth rates vary with pressure gradient (as we know to be the case); but it could also be that breakdown is not sufficiently

concentrated. Spot-hunting could throw light on the question, and seemed to receive the warm support of all assembled.

Hodson's catalogue of the woes that beset turbomachinery fluid dynamicists - harsh environment, high curvature, three-dimensionality, awkward Reynolds numbers and so on - were partly illustrated by Wisler (GE Aircraft Engines), who showed hot film traces from a cascade facility depicting the whole range of phenomena from transition after tripping through attachment, relaminarisation and separation in a bubble, followed once again by transition and reattachment! The qualitative effects are so striking that it is not necessary to have the hot film properly calibrated to read shear stress. Ting Wang (Clemson) finds that acceleration may delay transition onset slightly, but can significantly affect transition zone lengths. Walker & Solomon (Tasmania) find that, on the stator blade of an axial compressor, with <u>adverse</u> pressure gradients that may cause laminar separation and intermittently turbulent reattachment, turbulent spots can appear periodically, following the growth of instability wave packets which <u>lag</u> behind wake passage. Lower FST does not alter the essential character of breakdown, confirming that FST is not the driving factor in adverse pressure gradients. Amidst all this talk of turbulence, Hodson reported some intriguing observations in a radial inflow turbine, where (in spite of relatively high Reynolds numbers and disturbance levels) the flow remains laminar or at best intermittent.

Hypersonics was on many people's minds but did not attract too much attention at the Workshop. Data obtained by Kimmel

(Wright-Patterson), including shadowgraphs and spectra, show that transition onset, as indicated by deviation of boundary layer parameters from laminar values, occurs where the second stability mode (clearly seen in the spectra) saturates. Whether this onset is accompanied by turbulent bursts of any kind is still not clear.

Modelling efforts appeared to fall into two classes: those that take explicit account of streamwise intermittency and those that do not. In the first class Fraser (Dundee) has a model that introduces new correlations for Narasimha's non-dimensional spot formation rate, and shows good agreement with Sharma's experiments on turbine blades, and the data of Abu-Ghannam & Shaw and Dhawan & Narasimha. Ashworth (Rolls Royce), after an assessment of surface film-gauge data under realistic conditions, concludes that transition in turbomachinery flows can be adequately modelled on the basis of Emmons's spots. Eli Reshotko (Case Western), reviewing models, said that the intermittency distributions of Narasimha, Arnal and Chen & Thyson all appeared viable, and that their use was now spreading surprisingly fast. PSE, in combination with e^n methods, was proving very useful in quiescent streams (FST < 0.4%), although Herbert found that if stability conditions were rapidly alternating, e^n could be a poor guide.

What about "by-pass"? There is a general tendency to use this word interchangeably with "high disturbance", but this was questioned at the Workshop. Are we on a by-pass if our hot-wire probes do not show TS waves? We cannot say yes, for Kendall can find TS waves in surface sensors when hot wires do not reveal them. Gaster thinks the TS mechanism may be operating even in the

absence of recognizable TS waves: the boundary layer may still be responding through a TS transfer function even if it has not picked out relatively pure TS waves. There is on the other hand the observation of Blackwelder (USC) that, when rigid particulates are introduced into a laminar boundary layer, turbulent spots arise from the disturbances due to the resulting wake that "scars" the boundary layer, rather than from the particulates themselves. Narasimha suggested that perhaps we should talk about a by-pass only if the mean flow were sufficiently modified to totally alter the transition mechanism. The issue was left hanging, and the participants found it convenient to ignore it for the moment, as they proceeded to talk of "weak" bypass (FST between 0.4% and 2%) and "strong" bypass (FST > 2%) as before (in spite of the fact that FST is not the dominating factor driving transition in adverse pressure gradient flows). By this definition all turbomachinery applications involve only by-pass routes: the disturbance environment is severe, with upstream blade rows often adding periodic wake-passage as another special dimension in disturbance space (as Hodson emphasized). Reshotko considered the weak bypasses the most dangerous, and advocated PSE and correlations to tackle them. For strong bypasses he added the $K\varepsilon$ equations. Max Platzer (Monterey) uses a modified Chen-Thyson model to compute flow past airfoils with separation bubbles, and finds that the incorporation of a transition model is crucial for predicting the bubble. Crawford (Texas at Austin) is developing two-equation models, especially for strong by-pass. Neither the Launder-Sharma models nor Crawford's mutliple time scale improvements seem able to predict the peak skin-friction and heat

transfer parameters that are now well known to occur towards the end of the transition zone. My own feeling is that differential models for transitional flow, without a built-in intermittency, cannot yet perform as well as properly constructed integral-type methods.

There is one silver lining in the cloud in turbomachinery fluid dynamics: blade Reynolds numbers are low enough to make direct numerical solution at full-scale feasible. Thus Manmohan Rai (now at NASA Langley) is able to simulate flow in a spatially developing boundary layer on a flat plate all the way to full turbulence. He finds rather narrow and elongated spot-like regions, more Kendall than Emmons, it seemed to me. Perhaps the rather narrow plate width (only about 130 momentum thicknesses) constrains turbulent regions. Neil Sandham (Queen Mary & Westfield), working with Kleiser at DLR, simulates the complete transition process in a channel: from the intermediate stages involving shear layers, their roll-up, lambda vortices, and, later on, sublayer streaks, ejections etc. The role of the Emmons spot in these simulations remains rather vague yet. But the question was raised, especially by Steve Robinson (NASA Langley), whether all the "captive" fluid mechanics produced on the computer is teaching us enough to be worthwhile? There was general agreement with Reshotko's proposal that DNS should now be done on a mission mode - planned like a flight experiment, say - and that the data must be easily available for interrogation by scientists through simple, well-understood procedures.

Transition control is in some sense an ultimate objective in

applications, but was not much discussed. Hodson thinks there may be optimum wake-passing frequencies from the point of view of the boundary layer. If wake impact triggers transition through wave packets, and a wake-induced turbulent slab conceals within itself a number of turbulent spots (as some experimental evidence suggests), alteration of wake impact frequency may change transition zone parameters in interesting ways. Nosenchuck & Brown (Princeton) are trying a more direct active method, using a wall-normal Lorentz force in the flow of an electrolyte past a flat plate. The Lorentz force is generated by a spanwise magnetic field acting on a streamwise current, and acts by inhibiting lift-up and bursting in the wall layer. Control is exercised through 'tiles' consisting of permanent magnet and stainless steel surface-mounted electrodes. Laser sheet views and velocity traces show dramatic changes when control is on; e.g. stresses are down by upto 90% at a Reynolds number of 1700 (based on momentum thickness), with an applied field of 1000 gauss and current density of 10 mA/cm^2.

On the last day of the Workshop, three working groups summarised their suggestions on what needs to be done. Apart from the question of where spots are born, there are many others that call for answers. Are celerities different from local free-stream velocities in pressure-gradient flows? Does spot growth occur by birth of offspring which amalgamate with the parent? How often do spots trigger the birth of other autonomous spots in the neighbourhood? How strong are spot-interaction effects? Do spot propagation parameters vary widely with pressure gradient? What

happens in strongly curved and 3D flows? And so on.

On the routes leading to spots, the respective roles of narrow Kendall-type wave packets and the wider Gaster-type needs to be clarified. The damped Klebanoff mode needs serious consideration as it may be important in both moderate and high free-stream turbulence. Is it enough to consider free-stream turbulence frozen, or does its evolution need to be accounted for? - this remained an open question.

Frank Smith made a strong plea for greater attention to the physics of the end game. This needs theory, computation and experiment to go hand in hand, but the manpower to do it is not visible. The rewards, on the other hand, are many, and include better transition models and greater understanding of the multiple roads to turbulence, and, eventually, more efficient transition management.

The meeting was one of the most rewarding and stimulating I have attended on transition, because it got two till-now distant communities together, put them in an isolated spot where interaction was easy, kept the group small enough for intimate discussions and organized a programme that had just the right pace - neither overcrowded nor leisurely. I wish there were more meetings like this one.